

Psychological Monographs

General and Applied

The Relation Between Possession of
Task-Relevant Information and
Attempts to Lead

By

John K. Hemphill
Pauline N. Pepinsky, Reuben N. Shevitz,
William E. Jaynes and Charlotte
A. Christner

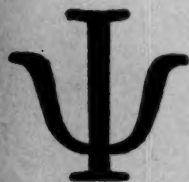
*Personnel Research Board
The Ohio State University*

Price \$1.00

No. 414
1956

Hemphill, Pepinsky, Shevitz,
Jaynes, Christner

Vol. 70
No. 7



Edited by Herbert S. Conrad
Published by The American Psychological Association, Inc.

Psychological Monographs: General and Applied

Combining the *Applied Psychology Monographs* and the *Archives of Psychology*
with the *Psychological Monographs*

Editor

HERBERT S. CONRAD

Department of Health, Education, and Welfare
Office of Education
Washington 25, D.C.

Managing Editor

LORRAINE BOUTHILET

Consulting Editors

DONALD E. BAIER
FRANK A. BEACH
ROBERT G. BERNREUTER
WILLIAM A. BROWNELL
HAROLD E. BURTT
JERRY W. CARTER, JR.
CLYDE H. COOMBS
JOHN F. DASHIELL
EUGENIA HANFMANN
EDNA HEIDBREDER

HAROLD E. JONES
DONALD W. MACKINNON
LORRIN A. RIGGS
CARL R. ROGERS
SAUL ROSENZWEIG
ROSS STAGNER
PERCIVAL M. SYMONDS
JOSEPH TIFFIN
LEDYARD R. TUCKER
JOSEPH ZUBIN

MANUSCRIPTS should be sent to the Editor.

Because of lack of space, the *Psychological Monographs* can print only the original or advanced contribution of the author. *Background and bibliographic materials must, in general, be totally excluded* or kept to an irreducible minimum. Statistical tables should be used to present only the most important of the statistical data or evidence.

The first page of the manuscript should contain the title of the paper, the author's name, and his institutional connection (or his city of residence). Acknowledgments should be kept brief, and appear as a *footnote* on the first page. No table of contents need be included. For other directions or suggestions on the preparation of manuscripts, see: CONRAD, H. S. Preparation of manuscripts for publication as monographs. *J. Psychol.*, 1948, 26, 447-459.

CORRESPONDENCE CONCERNING BUSINESS MATTERS (such as subscriptions and sales, change of address, etc.) should be addressed to the American Psychological Association, Inc., 1333 Sixteenth St. N.W., Washington 6, D.C. Address changes must arrive by the 15th of the month to take effect the following month. Undelivered copies resulting from address changes will not be replaced; subscribers should notify the post office that they will guarantee third-class forwarding postage.

COPYRIGHT, 1956, BY THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

Psychological Monographs: General and Applied

The Relation Between Possession of Task-Relevant Information and Attempts to Lead¹

John K. Hemphill, Pauline N. Pepinsky, Reuben N. Shevitz,
William E. Jaynes, and Charlotte A. Christner^{2,3}

Personnel Research Board, The Ohio State University

I. INTRODUCTION

A. GENERAL STATEMENT OF THE PROBLEM

THE present experiment is the first of a projected series concerned with the prediction of individual attempts at leadership in small groups. Specifically, the study to be reported is an inquiry into the extent to which attempted leadership acts are a function of the possession of task-relevant information. "Attempted leadership acts" are acts accompanied by an intent to initiate structure-in-interaction during the process of solving a mutual problem. The central hypothesis under test is drawn from a

theory of leadership in small groups proposed by Hemphill (23, Appendix A). In general, the current project is restricted to the problem of defining and describing the motivational factors underlying individual attempts to lead. Whether such attempts are "successful" (i.e., accepted or followed by the group members) or "effective" (i.e., do, in fact, lead to correct problem solution) are at this stage important but irrelevant questions.

B. RELEVANT CONCEPTS AND POSTULATES FROM A PROPOSED THEORY OF LEADERSHIP IN SMALL GROUPS

The hypothesis tested in the present experiment makes use of selected con-

¹This study was conducted under Contract N6ori-17 T.O. III NR 171 123 between the Office of Naval Research and the Research Foundation of the Ohio State University. The opinions presented are those of the authors and should not be regarded as having the endorsement of the Department of the Navy. The research is a part of The Ohio State Leadership Studies, which were initiated in 1946 by Carroll L. Shartle and are planned as a ten-year program of research on leadership and administrative problems in military, business, industrial, educational, and civilian governmental organizations.

This paper is based on a technical report submitted to the Office of Naval Research which bears a similar title (see 23). Certain materials, e.g., a complete statement of the theory and sample forms, have not been included in the present paper in order to reduce printing costs. The original report is available through inter-library loan from The Ohio State University.

²The authors are grateful to Dr. Harold Garfinkel, now of the Department of Sociology, University of California at Los Angeles, for his invaluable advice in the initial planning stages of the experiment. They wish to thank Dr. Delos D. Wickens of the Department of Psychology, The Ohio State University, for his cooperation in securing subjects.

Acknowledgment is due also to Mr. Howard J. Ehrlich for his service as an observer, to the students who served as subjects in the experiment, and to Mrs. Arlene Bargar, Mrs. Ruth Ann Young, and Miss Evelyn Harris for their conscientious clerical assistance.

³John K. Hemphill is now with the Educational Testing Service, Princeton, New Jersey; Reuben N. Shevitz is with E. I. Lilly & Company; William E. Jaynes is with the United States Army.

cepts central to the theory and is derived from theorems that stipulate the variables upon which attempts to lead are contingent. The key concepts employed are those of "mutual problem," "interaction," "structure-in-interaction," and "the attempted leadership act." These concepts are defined as follows:

1. "A *mutual problem* is a situation involving two or more individuals, each of whom perceives the situation as dissatisfying, and that requires some behavior on the part of the other if either (any) is to be satisfied" (23, p. A-2).
2. "Interaction is defined as a series of reciprocal acts involving two persons, where the intentions governing the acts are influenced by acts, or expectations of the acts, of the other" (23, pp. A-6, A-8).
3. "Structure-in-interaction is defined as a consistency in behavior occurring during interaction that permits the prediction of the behavior that will occur in future interaction" (23, p. A-8).
4. "*Attempted leadership* acts are acts accompanied by an *intention* of initiating structure-in-interaction of others during the process of mutual problem solution" (23, p. A-18).

The theory holds that "the motivational variables that determine whether a given individual will attempt a given leadership act lie in two major areas. The first area involves need-deprivation variables. . . . The second area involves cognitive estimates or expectations of the individual concerning the probable consequence of the act of leadership he is considering attempting" (23, p. A-26). Within the second area "the possession of relevant information (perhaps not available to other group members)" is specified as a variable that contributes to the individual's ability "to follow clearly the probable consequence of the act under consideration" (23, p. A-27). Under this circumstance he may attach a relatively high probability of effectiveness to an act that he is considering. "On the other hand, an individual will tend to estimate a low probability of effectiveness of a course of action that appears novel

with respect to his previous experience and about which he is unclear and confused as to its likely consequences" (23, p. A-27).

C. RELEVANT RESEARCH AND THEORY

Previous research on leadership and group behavior has been summarized and discussed elsewhere (14, 34, 26, 18), and Roseborough (31) has prepared a recent survey of experimental studies with small groups. It will not be necessary, therefore, to review here the large body of accumulated research, but it is appropriate to point to certain characteristics of other studies that have particular pertinence in the present context.

First, and of special significance, is the virtually complete lack of attention given to the problem of attempted leadership. A number of studies involving an apparently similar notion have had to do with the concept of "emergent" leadership as occurring in a "leaderless" group situation—e.g., see Ansbacher (1), Gibb (16), Bass (3), and Carter, Haythorn, Shriver, and Lanzetta (9). It is clear, however, from the published descriptions of the operations by which the behavior of emergent leaders has been assessed, as opposed to those used to measure attempts at leadership, that the terms *emergent* and *attempted leadership* are not identical. The difference lies in the fact that, in the former instance, leadership behavior, however defined, has been measured in retrospect, in the sense that only after a "leader" has emerged, does his behavior become a phenomenon of interest to the observer. In the latter case, the focus is upon conditions and variables that may motivate an individual simply to *try* to lead, without any regard for whether he does later, or ever, "emerge" as a leader, either in the eyes of the group members or of any outsiders.

Nevertheless, it should be granted that an undetermined proportion of an individual's attempts to lead may be at least partly responsible for his subsequent recognition as a leader. It is plausible, therefore, to assume that there is some degree of communality between what would be viewed by one observer as leadership behavior and by another as attempted leadership only. But in each case a different question is raised about that behavior: on the one hand the problem is to determine the factors related to different *leader* behaviors (taking for granted that a leader has been identified); on the other hand, the problem is to find out what variables are associated with *any* group member's attempts to lead.

An exception is the recently published monograph by Bass, Wurster, Doll, and Clair (4).

This study of situational and personality factors in leadership among sorority women makes use of a concept of attempted leadership. In this instance, however, attempted leadership is defined simply as the amount of participation and is treated as one of a series of measures obtained through employing the leaderless group discussion as a situational test. In view of the more restrictive definition of attempted leadership formulated by Hemphill, it is difficult to make a meaningful comparison between the present research and the investigation by Bass and his associates.

A second point of relevance to the formulation of the hypothesis tested in the current investigation is the recurrent finding, commented upon by Jenkins that "leaders in a particular field need and tend to possess superior general or technical competence or knowledge in that area" (26, p. 75). As Gibb (15, p. 22) has noted, Caldwell reported as early as 1920 that his study of nominated high school leaders had led him to conclude that one of the most important characteristics ascribed to these individuals was the possession of "practical knowledge relative to the situations for which they were chosen as leaders" (6). Although attention has been given to the possession of special knowledge or training as an attribute of *acknowledged* leaders, the question does not appear to have been asked as to whether, given an individual who possesses special training or information, it then can be predicted that he will *attempt* to lead.

A third frequently stated conclusion, congruent with the one above and also cited by Jenkins, is that "leadership is specific to the particular situation under investigation" (26, p. 75), also see Hemphill (21). Recently, some qualifications of this categorical statement have been introduced by the studies of Carter, Haythorn, Meirowitz, and Lanzetta (8) and Bell and French (5). But their findings serve not so much to contradict the specificity hypothesis as to underscore the complexity of the phenomena under scrutiny. The fact remains that the degree of specificity that has seemed to characterize the behavior of leaders and the variables associated with leadership has been one of the most difficult obstacles to surmount in the attempt to derive systematic generalizations in the field of leadership study.

Recognition of this state of affairs has led to an expressed need for more knowledge about the effects of particular situational variables upon group behavior. Roseborough says, "Such knowledge is vital to every small-group study since the results are conditional on these factors. Too often situational variables have been uncontrolled and unspecified in the design of experiments" (31, p. 297). And Heise and Miller have urged the importance of defining the sig-

nificant dimensions of the problem itself, citing as possibilities "the degree to which the problem is structured . . . the uniqueness of the solution, *the initial distribution of information* (italics ours), the number of decisions that some one must make," etc. (20, pp. 334-335).

With respect to the conceptual views underlying current research on leadership in small groups, both Katz (28) and Smith (33) have remarked upon the shift from the earlier concern with the "traits of leaders" to a relational concept of leadership, or what Gibb has referred to as an "interactional theory" (15). Brief mention will be given to two such efforts toward systematic formulation of some principles of group behavior that seem to have particular aspects in common with the theory proposed by Hemphill. The first is Deutsch's theory of cooperation and competition (10, 11, 12). Specifically, he has linked the concept of group with the concept of cooperation in that the cooperation situation is viewed as equivalent to the sociological group. "A sociological group exists (has unity) to the extent that the individuals composing it are pursuing promotively interdependent goals" (11, p. 150). The phrase "promotively interdependent goals" implies that the attainment of any group member's goal is dependent upon every other group member attaining his goal. There appears to be a close analogy between Hemphill's concept of the activity of group members engaged in solving a "mutual" problem and Deutsch's concept of the sociological group defined by the pursuit of promotively interdependent goals. Increased knowledge of the conditions and variables associated with cooperative behavior in groups may have implications for the study of attempted leadership in groups working toward the solution of mutual problems.

A recent paper of Newcomb's (30) sketches a theory of communicative behavior, which reveals some similarity of conceptualization to Hemphill's formulation. Newcomb has defined the communicative act, the unit of interaction, as "any observable behavior by which information, consisting of discriminative stimuli, is transmitted from a human source to a human recipient" (30, p. 141). It is his proposal that human social behavior "be studied in terms of the conditions and consequences of varying communicative acts" (30, p. 141).

It is possible that attempted leadership acts, if regarded as a class of communicative acts, could be accounted for in such a system as Newcomb suggests. Thus, in the case of the approaches to the study of group behavior proposed by Deutsch, and by Newcomb, certain parallels to the investigation of attempted leadership are discernible.

D. THE HYPOTHESIS TO BE TESTED

The central hypothesis tested in this experiment is drawn from the selected portions of the theory given in Section I. B. The postulated relationships have led to the formulation of the proposition that the possession of relevant information, which is not available to others, will be significantly related to the frequency with which a given individual will attempt acts of leadership during

the course of the problem-solving activity of a given set of persons. Stated in null form, the specific hypothesis submitted to experimental test is as follows: in a mutual problem-solving situation the frequency of attempts to lead by those individuals who possess task-relevant information is not significantly different from the frequency of leadership acts attempted by individuals who possess nonrelevant information.

II. PROCEDURE

A. GENERAL DESCRIPTION OF THE EXPERIMENTAL DESIGN

The experiment was conducted according to a factorial design. The major dependent variable was the number of attempted leadership acts that were tallied for each of four group-members during the time that they worked on each of four tasks. A time limit of 15 minutes was set for each task and the groups proceeded from one task to the next without any appreciable delay. The principal independent variable was the task-relevant information supplied to each member during a training period that preceded the experimental sessions.

During the training session each group member was provided with a booklet of information, which he was permitted to study for forty minutes. Each of these booklets, made up and coded in advance, contained four sets of informational material of approximately equal length, but of varied relevance to the particular tasks. For each task two sets of information were prepared, one clearly more relevant and the other less relevant to the solution of the problem. The prearranged composition of the booklets was such that one group member's booklet might contain three sets of irrelevant and one set of relevant information,

while another's contained two sets of relevant and two of irrelevant information, etc.

The four tasks used in the experiment were an Assembly task (Task A), a Strategy problem (Task S), a Construction task (Task C), and a Discussion problem (Task D). Sixteen groups of four members each served as subjects in the experiment. They began their first task as unacquainted individuals and as members of "leaderless" groups. Table 1 illustrates the basic design. The order in which the tasks were presented and the number of individuals possessing relevant information are given for the first eight sessions, or one-half the total number. During the remaining eight sessions an experimental replication was provided by repeating the basic design.

The subjects were selected from among the male university students who were enrolled in an elementary psychology course. The selection procedure involved choosing volunteers who were found, on the basis of a screening test, to be relatively naive in respect to the types of task used in the experiment.

At the beginning of the experimental sessions the subjects were told that they would be in competition with other groups for monetary prizes to be awarded

TABLE 1
BASIC DESIGN OF THE EXPERIMENT

Group I						Group II						Group III						Group IV					
Trial	Task	Individual				Trial	Task	Individual				Trial	Task	Individual				Trial	Task	Individual			
		1	2	3	4			5	6	7	8			9	10	11	12			13	14	15	16
1st	A	N	N	N	N	1st	C	R	R	R	R	1st	S	N	N	N	N	1st	D	R	R	R	R
2nd	S	R	R	R	R	2nd	D	N	N	N	N	2nd	A	R	R	R	R	2nd	C	N	N	N	N
3rd	D	R	R	R	R	3rd	A	R	N	N	N	3rd	C	R	N	N	N	3rd	S	N	N	N	N
4th	C	R	R	R	N	4th	S	A	R	R	N	4th	D	R	R	N	N	4th	A	R	R	R	N
Group V						Group VI						Group VII						Group VIII					
Trial	Task	Individual				Trial	Task	Individual				Trial	Task	Individual				Trial	Task	Individual			
		17	18	19	20			21	22	23	24			25	26	27	28			29	30	31	32
1st	S	R	R	R	R	1st	D	N	N	N	N	1st	A	R	R	R	R	1st	C	N	N	N	N
2nd	A	N	N	N	N	2nd	C	R	R	R	R	2nd	S	N	N	N	N	2nd	D	R	R	R	R
3rd	D	R	N	N	N	3rd	S	R	N	N	N	3rd	D	R	N	N	N	3rd	A	R	N	N	N
4th	C	R	R	R	N	4th	A	R	R	R	N	4th	C	R	R	N	N	4th	S	R	R	R	N

Tasks:

- A. Assembly problem (Electronic circuit)
 S. Strategy problem (Peggy game)
 C. Construction problem (The Toy, airplane design)
 D. Discussion problem (Election results)

Information:

- N = possess information
 not relevant
 R = Possess information
 relevant

on the basis of their task performance. The four groups with the best task performance were to be given cash awards.

No restriction was placed on the subjects' freedom to move about and converse with each other. The fact that subjects could communicate freely with one another precluded the possible differential effect upon the incidence of attempts to lead of position within a communicative network. See Leavitt (29, p. 46).

B. THE DEPENDENT VARIABLE

1. Observational Procedure

As noted above, the observed frequency of attempted leadership acts constituted the dependent variable in this experiment. The particular procedures followed by the observers were governed by the requirement that their observations and inferences be linked directly to the theoretical definition of the attempted leadership act as "an act ac-

companied by an intent to initiate structure-in-interaction of others during the process of mutual problem solution."

Reference to such systems of categorization of observed group behavior as those developed by Bales (2) and Carter, Haythorn, Meierowitz, and Lanzetta (7) suggested that certain of their categories might include behavior similarly codified. Specifically, Bales's fourth category ("gives suggestion, direction, implying autonomy for others" (2, pp. 181 ff.), or Carter's categories 25 and 26 ("proposes course of action for self" and "proposes course of action for others" (7, p. 259) might plausibly be expected to subsume many "attempted leadership acts."

But without a subsidiary methodological study, which is outside the scope of this experiment, it would not be possible to say to what extent these labels do have common behavioral referents. As Heyns and Zander have emphasized, a decision about the applicability of a category

system "depends upon the purposes of the investigator and the theoretical framework within which he is working" (25).

Accordingly, in an attempt to achieve maximum clarity and precision, an Observer's Manual was developed especially for use in the current research. From the theoretical definition of the attempted leadership act a set of procedural rules was derived that specified those acts clearly included in the system and to be scored, and those acts clearly excluded and not to be scored. Thus, the category system comprised what Guetzkow has called a "sieve code" (17, p. 48) in that only the behaviors falling into a single major category were tabulated; all other behavior was ignored. The manual also outlined procedures for categorizing acts that were "marginal" in the sense that scoring them required relatively complicated inferences on the part of the observer. Three trained observers independently tabulated attempted leadership acts.

One of these observers had not participated in the earlier development of the scoring rules, was unfamiliar with the underlying theory, and was trained solely by reference to the manual in its final form. This observer was introduced in

order to determine whether the manual had made the concept of the attempted leadership act sufficiently communicable to yield an adequately reliable measure.

2. Reliability of Observations

There is no generally satisfactory solution to the problem of assessing agreement among observers in small-group experimentation. For the purpose of this experiment, the intraclass correlation (r) technique (27, p. 230 ff.), appeared to provide an adequate means of measuring interobserver consistency. This technique was applied to obtain, first, a measure of over-all agreement within the set of three trained observers, and, second, separate measures of agreement within each of the three pairs of observers. The scores used in this analysis were the distributions of the total number of attempted leadership acts assigned by each observer to each of the four individual subjects during every fifteen-minute period of the sixteen sessions. Thus, there were 256 separate scores for each observer. The obtained correlations are presented in Table 2.

Particularly in view of the fact that the observers were required to draw inferences with respect to the presence

TABLE 2
INTRACLAS CORRELATIONS AMONG TRAINED OBSERVERS' TALLIES OF ATTEMPTED LEADERSHIP ACTS

Observer set	Source of variation	Sum of squares	<i>df</i>	<i>F</i>	<i>r</i>
A+B+C	Between sets	25,388.69	255	15.69**	.83
	Within sets	3,250.67	512		
A+B	Between pairs	19,171.38	255	15.34**	.88
	Within pairs	1,254.00	256		
A+C	Between pairs	17,428.37	255	9.36**	.81
	Within pairs	1,870.00	256		
B+C	Between pairs	15,391.88	255	8.81**	.80
	Within pairs	1,752.50	256		

** Significant at .01 level.

of an "intent" on the part of the group members, an over-all correlation of the magnitude of .83 was considered adequate for the purpose at hand. The highest intrapair correlation, .88, as might have been expected, occurred between the two observers who had participated in the design of the experiment and the development of the manual, and who were most familiar with the theoretical framework. Nevertheless, there was only a relatively small and obviously not significant difference between this correlation and the separate measures computed for each of these observers paired with the third observer, "C," trained with reference to the manual only. These correlations of .81 and .80 supported the contention that the concept of the attempted leadership act as defined theoretically could be given behavioral referents subject to reliable observation.

3. *The Attempted Leadership Act and a Naive Concept of Leadership*

For eleven of the sixteen sessions a fifth subject was available. The fifth subject was employed as a naive observer and was instructed simply to rate the group members on "amount of leadership." He worked behind a one-way vision mirror and made a judgment every 5 minutes.

Product-moment correlations were computed between (a) the sum of the naive observers' ratings for each of the four subjects during every 15-minute period of the eleven sessions, and (b) the total of the attempted leadership acts ascribed to each subject by the trained observers during the same time periods. The correlation obtained was .47 ($N = 176$). It would appear, therefore, that, while the experimenter's concept of attempted leadership is not entirely divorced from the concept of leadership held by the layman, the two notions show

only a slight correspondence. An important qualification to be noted is that the rigorous restriction of the experimenter's concern to attempted leadership acts, without regard for their "success" or "effectiveness," had an unknown and possibly significant effect upon this measure of association.

C. THE EXPERIMENTAL CONTROLS

Certain methods of experimental control were introduced to insure the experimental manipulation of relevant information as the independent variable and to restrict the influence of extraneous variables. These controls were introduced in several ways: (a) in the development and selection of the experimental tasks to conform with certain specified criteria; (b) in the selection of subjects who were without previous knowledge directly relevant to the tasks; (c) in the employment of a pre-experimental training session held under controlled conditions to provide specified individuals with information relevant to particular tasks; (d) in the establishment of an external judgment criterion used to test the discriminability of the "relevant" and "irrelevant" sets of information; and (e) in the use of pre- and postexperimental testing procedures that provided a means both of relating amount of information to task performance and of determining the extent to which information was unevenly distributed throughout the experiment.

1. *The Tasks*

Four experimental tasks were devised, the characteristics of which were designed to facilitate the manipulation of task-relevant information and to maximize the likelihood that attempts to lead would be elicited:

a. *Assembly task.* The subjects were required to assemble a simple electronic circuit to conform to a prescribed schematic diagram. The

materials included a box containing a large number of assorted components (resistors, condensers, coils, etc.) and an 18" \times 24" sheet of cardboard upon which were drawn the schematic diagram and a set of squares. These squares corresponded to the elements of the schematic diagram, and were connected by straight lines, which represented the wires connecting the component parts. The subjects were told to assemble the circuit by selecting the correct parts from the box and placing them in the proper squares.

b. Strategy problem. This task involved participating in a game of peggity, which is played by inserting pegs into rows of holes on a large pegboard, with the objective of scoring by getting eight pegs in an unbroken row. The four group members played as a team against the experimenter, who played according to a fixed set of rules. Four pegs were placed on each move, each group member placing one. The team was permitted to discuss its moves before making them, but was required to reach agreement about the placement of all four pegs before each play. The team initiated the play by having the first move, and play continued until the expiration of the time limit.

c. Construction task. The group was assigned the task of constructing a model airplane from a set of commercially available materials called The Toy. Included in the materials were large square and triangular panels made of heavy colored paper, a number of wooden dowels, and a supply of pipe cleaners used to fasten the parts together. The group was instructed to build a model large enough for a four-year-old child to get inside, and was told that its design would be scored against one given in the manufacturer's blueprint.

d. Discussion problem. This problem required the group to arrive at an estimate of the total number of votes cast for the Republican presidential candidate in the state of Oregon in 1940. The group was asked to deliberate and arrive at a consensus approximating the official figures as closely as possible. A list of four facts about Oregon was made available to the groups.

2. Checks on Relevant and Irrelevant Information

In order to determine whether the prepared sets of information could be distinguished, on other than an a priori basis, as "relevant" and "irrelevant" to the tasks, another kind of check was introduced. After being provided with brief descriptions of the tasks, six judges made an independent assignment of each

of these sets of written material to either the "relevant" or "irrelevant" category. By application of the chi-square technique, using the exact treatment appropriate for a two-by-two table as given by Fisher (13, pp. 100-102), a probability of .001 was obtained for three of the tasks, and of .04 for the fourth. Thus, for three tasks, in only one instance in a thousand would such "accuracy" in sorting be expected to occur by chance alone. Failure to categorize "accurately" by one judge, as occurred with Task A (Assembly), would be predicted by chance in four instances in a hundred. This appraisal supported the assumption that the information would be distributed in accordance with the experimental design.

3. Task Performance

The relation of performance to possession of relevant or irrelevant information was obtained by testing the significance of differences between the performance scores of those group members who possessed relevant information and those who did not. For this purpose, since the nature of the tasks had prevented the determination of individual performance scores, the group performance scores were assigned to individual members.

The group's score was treated as the score received by each subject, and *t* values were computed to test the significance of the difference between the means of the scores of the individuals who had relevant information and those who had irrelevant information. The results of these tests for each of the four tasks are shown in Table 3. On two tasks, the Assembly task and the Construction task, the individuals who possessed relevant information were members of groups whose performance scores were significantly higher than those of

TABLE 3
MEAN TASK PERFORMANCE OF SUBJECTS
POSSESSING RELEVANT AND
NONRELEVANT INFORMATION
($N_1 = N_2 = 32$)

Task	Relevant		Non-relevant		<i>t</i>
	M	σ	M	σ	
Assembly	6.22	2.14	2.16	2.52	6.88**
Strategy	2.94	1.88	3.09	2.25	-.29
Construction	6.59	1.81	4.53	2.20	4.12**
Discussion	3.53	2.60	4.84	2.98	-1.82

** Significant at .01 level.

individuals possessing irrelevant information. Although significant differences were not obtained with respect to the Strategy and Discussion tasks, the difference found in the case of the Discussion task did approach significance and occurred in a reversed direction.

While this analysis had the advantage of utilizing all the available data, it had an obvious disadvantage in that, on two of the problems, each group contained some members who had relevant information and some who did not. Thus some group scores were assigned both to individuals who possessed relevant information and to individuals who possessed irrelevant information. Furthermore, this procedure combined scores for individuals who were members of groups that were run under different information conditions; the effects on performance of possession of relevant information might have varied with the number of group members who possessed such information. Accordingly, the same sort of analysis was made again, using only the first two trials in which the members all had received the same (i.e., either all relevant or all irrelevant) information. Except in the case of the Discussion problem, the results of this additional analysis did not differ substantially from those reported in Table 3. The *t* value (3.87)

obtained for this task is significant at the .01 level. The direction of the difference indicates that groups who had irrelevant information scored higher (i.e., performed better) than groups who had relevant information.

4. Distribution of Information

Since the major hypothesis was concerned with relevant information as it was possessed by different individuals engaged in solving different tasks, it was important to determine whether such information actually was unequally distributed at the outset and remained so at the end of the experimental session. Accordingly, two tests were administered to all group members: one, a reading comprehension test, was given immediately following the training session; and the other, a retention test, was given immediately following the experimental session. These two instruments, composed of multiple-choice items, were designed to measure the relative amount of information acquired and retained by the individual subjects.

An analysis of the differences in scores on the comprehension (pre-experimental) and retention (postexperimental) tests showed that, for all tasks, those who had received relevant information scored significantly higher than those who received irrelevant information. Table 4 shows the means and standard deviations of scores received by individuals in the two categories and the *t*'s obtained when the differences between these categories were tested.

We may interpret the results of these information tests as indicating that the individuals who had been given the relevant information to read possessed superior knowledge both at the beginning and at the end of the experimental session.

TABLE 4
MEANS OF THE TWO GROUPS FOR RELEVANT ITEMS ON COMPREHENSION TEST, RELEVANT ITEMS
ON RETENTION TEST, AND NONRELEVANT ITEMS ON COMPREHENSION TEST
($N_1 = N_2 = 32$)

Test	Task	Relevant		Nonrelevant		
		M	σ	M	σ	<i>t</i>
Comprehension Relevant	Assembly	2.78	1.07	1.72	1.17	3.79**
	Strategy	2.94	1.44	1.13	1.04	5.84**
	Construction	3.00	1.28	1.28	1.14	6.03**
	Discussion	2.84	.98	1.31	1.00	6.12**
Retention Relevant	Assembly	3.28	1.11	2.31	.93	3.73**
	Strategy	3.34	1.43	2.13	1.16	3.67**
	Construction	2.97	1.18	1.91	1.06	3.79**
	Discussion	2.91	1.25	1.34	.66	6.28**
Comprehension Nonrelevant	Assembly	1.84	1.17	2.07	1.45	-3.42**
	Strategy	.91	.82	2.69	1.23	-6.59**
	Construction	1.88	.94	4.22	.94	-9.75**
	Discussion	1.28	.89	2.72	1.22	-5.33**

** Significant at .01 level.

It appears also, that there was improvement in the test scores from the initial test (reading comprehension) to the final test (retention). In the cases of the Assembly task and the Strategy task *t* tests of the significance of the increases in test scores yielded values of 3.55 and 3.53 respectively (.01 level). In the cases of the Construction task and the Discussion task the *t*-test values were 1.22 and .27, which are not statistically significant. If all four tasks are taken together in one test of the significance of the increase in scores, a *t* value of 4.13 is obtained, indicating a significant improvement in knowledge about the test items after the experimental session. The higher scores on the retention test cannot be attributed to the difficulty level of the items in the tests, since the items of relevant information used in the reading comprehension test and the retention test for the subjects tested during the first half of the experiment were interchanged between the two tests during the second half of the experiment.

In summary, controls instituted

through the factorial design and the experimental procedures operated in the following major ways:

1. Controls imposed by the observers' procedure assured the effective measurement of the dependent variable, attempted leadership acts.

2. Selection of novel tasks and procedures designed to eliminate subjects who possessed prior task-relevant information assured that information would be distributed in accord with the factorial design.

3. The classification by six independent judges of the information materials into a relevant or irrelevant category supported the assumption that information would be distributed initially in conformance with the factorial design.

4. Comprehension and retention tests administered before and after the experimental session indicated that more relevant information was possessed by subjects who had received relevant articles than by those who had received irrelevant articles.

5. An analysis of performance scores

indicated that for two tasks, Assembly and Construction, subjects possessing relevant information were members of groups making significantly higher performance scores than groups containing members with nonrelevant information.

6. The factorial design permitted the removal or control of variables such as task order, type of task, and individual differences in "leadership ability" in determining the effects of possession of information on attempts to lead.

III. RESULTS

A. THE DATA

In the course of the study three major classes of data were collected: (a) group performance scores for each of the four tasks and for the sixteen groups, (b) individual scores on information tests administered before and after the experimental sessions, and (c) frequency distributions of tallies of individual attempts to lead under the various experimental conditions. Analyses of the first two sets of data, to determine in one instance the relation between task performance and possession of relevant information, and in the second instance to check upon the manipulation of the independent variable, have already been reported (see Section II, C, 3, 4). The third class, however, comprising observers' tallies of attempted leadership acts, constituted the data of primary interest. The average number of attempts to lead during a 15-minute work period, as recorded by the three observers for each individual, ranged from 0.0 to 36.7. It is with reference to this particular set of data that we have examined the principal hypothesis of the study; namely, that an individual will attempt acts of leadership with a greater frequency when he does, than when he does not, possess task-relevant information. Through application of the analysis of variance, the effects of the experimental variables and the effects of their interaction on attempted leadership scores have been studied in detail. The obtained results

will be presented in this section.

Before the analysis could be undertaken, it was necessary to determine whether two assumptions underlying the use of the analysis of variance could be met. In respect to the first assumption of normality of distribution, inspection of the raw data revealed a marked skewness with disproportionate representation in the low frequencies. By using as a transformation the square root of the sum of the attempted leadership acts noted by the three observers, a distribution not significantly different from the normal curve was obtained ($p > .30$). The integer values of the transformed scores then were used as a measure of an individual's attempted leadership acts. These attempted leadership "scores" comprised the basic data for all the subsequent analyses. In regard to the second underlying assumption of the analysis of variance, i.e., homogeneity of subclass variance, Bartlett's test (27, pp. 83-85) yielded a probability of .89, supporting the assumption of homogeneity of variance within the 128 pairs of scores.

The variance analysis focused upon the assessment of group differences, task differences, and possession of information, all treated as main sources of variation. It also was possible to examine the first-order interactions of these potential sources of variation. In addition, two subpartitions of the variance associated with group differences were possible. One of the subpartitions separated those

groups that began the sequence of four tasks with relevant information from those that began the sequence without relevant information. The second partition was based upon the four different orders in which the tasks were presented to the groups.

B. ANALYSIS

1. The Analysis of Variance

Table 5 presents the analysis of the variance of the attempted leadership scores in which tasks, groups (initial information and task sequences), and information represent the partitions by main potential sources of variation. In the original tests of significance (F_1), the interactions of Group \times Tasks and Information \times Tasks were found to be not significant. Accordingly, the sums of squares associated with these sources were combined with the residual to pro-

vide a new error term for a second series of tests of significance (F_2). The analysis is discussed in terms of the findings obtained from the second series of tests of significance.

From Table 5 we see that the variance sources, task differences and group differences, have a significant effect upon the attempted leadership scores. The effect of information on the attempted leadership scores appears indirectly in first- and second-order interactions involving group differences.

a. Task differences. The mean attempted leadership scores for the four tasks were as follows: Assembly, 2.67; Strategy, 5.19; Construction, 3.73; and Discussion, 2.77. The Strategy task elicited by far the largest number of attempted leadership acts. The fact that this task required group consensus on each of a series of moves fostered vigor-

TABLE 5
ANALYSIS OF VARIATION IN ATTEMPTED LEADERSHIP SCORES ASSOCIATED WITH GROUP DIFFERENCES (INITIAL INFORMATION AND TASK SEQUENCES), TASK DIFFERENCES, AND RELEVANCE OF INFORMATION

Sources of variation	df	Variance	F_1	F_2
Between tasks	3	87.36	40.26**	45.98**
Between groups	15	7.63	3.52**	4.02**
Between initial information	1	26.91	12.40**	14.16**
Between Task Sequences	3	5.79	2.66*	3.05*
Initial Information \times Task Sequences	3	1.02	—	—
Remaining group variation	8	8.39	3.87**	4.42**
Between information	1	1.13	—	—
Groups \times Information interaction	15	4.30	1.98*	2.26**
Information \times Initial information	1	8.62	3.97*	4.54*
Information \times Task Sequences	3	5.57	2.57	2.93*
Information \times Initial Information \times Task Sequences	3	9.79	4.51**	5.15**
Information \times Remaining Group Variation	8	1.22	—	—
Groups \times Tasks Interaction	45	.93	—	—
Information \times Tasks Interaction	3	.93	—	—
Residual (for F_1)	173	2.17		
Combined residual (for F_2)	221	1.90		
Total	255			

* Significant at .05 level.

** Significant at .01 level.

ous interaction and many proposals of possible plays.

b. Group differences. Group differences, which were significant in their effects on the attempted leadership scores, were made more specific in the detailed analyses, involving the subpartitions of the group differences. The eight groups that began the series of four tasks with relevant information on the first task had an average attempted leadership score of 3.91 as compared with 3.27 for the eight groups that began without relevant information on the first task. It appears that initial group experiences associated with the possession of relevant information during the first task established a set toward attempting to lead.

The attempted leadership score of the group also was dependent upon the sequence in which the four tasks were presented. When the sequence began with the Discussion task (mean = 3.15), the groups attempted fewer leadership acts than when the sequence began with the Assembly task (mean = 3.72), the Strategy task (mean = 3.64), or the Construction task (mean = 3.84). The difference between the sequence headed by the Construction task and the sequence beginning with the Discussion task is significant at the .01 level. These two tasks differed in the degree to which they facilitated social interaction. While the Discussion task required some group discussion, unacquainted group members showed a strong tendency to work as individuals with the factual information supplied them. On the other hand, the Construction task required coordinated use of materials and permitted active participation by all members throughout the 15 minute interval.

c. Interaction effects. Statistically significant results bearing on the information variable are observed in the interac-

tions involving group differences and the information variable. The interaction of Information \times Initial Information is significant beyond the .05 level. Inspection of average attempted leadership scores for the four combinations involved in this interaction suggests that the type of information possessed on the initial task is particularly important in its influence on the number of leadership acts subsequently attempted under the condition of possessions of nonrelevant information. The average attempted leadership scores for the four conditions involved in this interaction are as follows:

1. For the subjects who had nonrelevant information on Trial 1, but relevant information on some other trials, the average of attempted leadership scores on trials on which they had relevant information was 3.52 ($N = 64$). The subjects and trials included in this average are as follows (refer to Table 1): Subject 1, Trials 2, 3 and 4; Subject 2, Trials 2 and 4; Subject 3, Trials 2 and 4; Subject 4, Trial 2; Subject 9, Trials 2, 3 and 4, etc.

2. For subjects who had relevant information on Trial 1, or on Trial 1 and on some other trials, the average of the attempted leadership scores on all trials on which they had relevant information was 3.80 ($N = 64$). The subjects and trials included in this average are as follows (refer to Table 1): Subject 5, Trials 1, 3 and 4; Subject 6, Trials 1, and 4; Subject 7, Trials 1 and 4; Subject 8, Trial 1; Subject 13, Trials 1, 3, and 4, etc.

3. For subjects who had nonrelevant information on Trial 1, or on Trial 1 and some other trials, the average attempted leadership scores on all trials on which they had nonrelevant information was 3.02 ($N = 64$). The subjects and

TABLE 6
AVERAGE ATTEMPTED LEADERSHIP SCORES
FOR DIFFERENT TASK SEQUENCES

Task sequence	Average attempted leadership scores	
	When possessing relevant information	When possessing nonrelevant information
Assembly first	4.00	3.44
Strategy first	3.69	3.59
Construction first	4.13	3.56
Discussion first	2.81	3.50

trials included in this average are as follows (refer to Table 1): Subject 1, Trial 1; Subject 2, Trials 1 and 3; Subject 3, Trials 1 and 3; Subject 4, Trials 1, 3 and 4; Subject 9, Trial 1, etc.

4. For subjects who had relevant information on Trial 1, but nonrelevant information on some other trials, the average of attempted leadership scores on trials on which they had nonrelevant information was 4.03 ($N = 64$). The subjects and trials included in this average are as follows (refer to Table 1): Subject 5, Trial 2; Subject 6, Trials 2 and 3; Subject 7, Trials 2 and 3; Subject 8, Trials 2, 3, and 4; Subject 13, Trial 2, etc.

The difference between the average attempted leadership scores for conditions 3 and 4 is significant beyond the .01 level. It appears that initial possession of *nonrelevant* information de-

presses the general level of attempted leadership scores under the condition of having nonrelevant information. On the other hand, initial experience with *relevant* information tends to stimulate attempts to lead even on later trials, when the subjects do not possess relevant information.

The interaction of Information \times Task Sequences is significant at the .05 level. This interaction is attributable largely to the fact that the expected relation between possessing information and attempting acts of leadership is found to reverse itself in the case of groups that began the sequence with the Discussion task. The data on which this interpretation rests are given in Table 6.

For those groups that began with the Discussion task, the average attempted leadership score for those possessing relevant information was found to be 2.81, as compared with a mean of 3.50 for those possessing nonrelevant information. It will be recalled that possession of information judged to be relevant was shown to be negatively related to performance on the Discussion task (see Section II, C, 3). Perhaps group members who have initial experiences with information that appears to be relevant, but actually may be confusing rather than helpful, come to distrust information that apparently is relevant to subsequent tasks.

TABLE 7
AVERAGE ATTEMPTED LEADERSHIP SCORES UNDER SIXTEEN EXPERIMENTAL CONDITIONS

Task sequence	Relevant information		Nonrelevant information	
	Initial relevant information	No initial relevant information	Initial relevant information	No initial relevant information
Assembly first	3.62	4.38	4.31	2.56
Construction first	4.44	2.94	3.35	3.44
Strategy first	4.25	4.00	3.81	3.31
Discussion first	2.87	2.75	4.25	2.75

The second-order interaction, Information \times Task Sequence \times Initial Information is significant at the .01 level. Second-order interaction effects are somewhat complex, but it seems worth while to examine this interaction in detail. Table 7 presents the average attempted leadership scores involved in this second-order interaction.

Apparently, the condition most conducive to attempting leadership acts had these two characteristics: (a) the group members possessed relevant information during their initial trial, and (b) they undertook the tasks in a sequence beginning with the Construction task. The least motivating condition for attempts at leadership seems to have been set by the possession of nonrelevant information, when the group began its initial task without relevant information, in a sequence beginning with the Assembly task.

A number of irregularities appear in Table 7. Note that for the task sequence beginning with the Discussion task, the experience of having relevant information on this initial task produced a larger number of attempted leadership acts under the condition of possessing nonrelevant information than under the condition of possessing relevant information. Again, this may be attributed to the inverse relation between the judged relevance of the information and task performance.

Note, also, under the condition of possessing relevant information, the case of the task sequence beginning with the Assembly task. In this instance initial possession of nonrelevant information has an effect opposite to that shown in the other task sequences. This may mean that inability to proceed with the task of assembling electronic components in the absence of clearly necessary information, produces a less depressing effect upon the group than similar experiences with more familiar tasks in which expectations of success are greater.

In general, the results of the analysis show no simple and direct effect of information upon the attempted leadership scores. However, the information conditions do become involved in the

TABLE 8
ANALYSIS OF THE VARIATION IN ATTEMPTED LEADERSHIP SCORES ASSOCIATED WITH GROUP DIFFERENCES, TASK DIFFERENCES, AND TRIAL DIFFERENCES

Sources of variation	df	Variance	F
Between groups	15	7.63	3.76**
Between tasks	3	87.36	43.03**
Between trials	3	3.54	1.74
Residual	234	2.03	
Total	255		

** Significant at .01 level.

interaction effects associated with other experimental conditions. *Task-relevant information as supplied in this experiment seemed to function as a catalyst in the leadership process, rather than as a principal agent.*

d. *Trial differences.* Since in the previous analysis Information was not found to be significant as a main source of variation, it was possible to disregard this experimental variable and test the significance of Trial differences on the attempted leadership scores. Group differences, Task differences, and Trial differences are available for this analysis, which is presented in Table 8.

We are interested in only the *F* ratio for between trials, which fails to meet the .05 level of significance. It appears, therefore, that there is no significant difference in the attempted leadership scores associated with whether the groups were working on their first, second, third, or fourth task.

e. *Individual differences.* Since there are no differences in number of attempted leadership acts from trial to trial, we may well consider the consistency of this measure for the individual. However, each trial represented a new task for the individual, and it has been demonstrated that there are significant differences in number of attempted

leadership acts as a function of Task differences. Accordingly, in order to eliminate this effect, which would tend to attenuate any measure of consistency, the scores for each task were standardized by converting them to McCall *T* scores. As a measure of the consistency with which individuals attempted to lead, an intraclass correlation coefficient was computed. The obtained intraclass correlation of .45 indicates that a consistency in attempts to lead may characterize the individual. However, a correlation of .45 is relatively small, suggesting that attempted leadership is not primarily a function of individual differences, but presumably is also related to situational characteristics. It should be noted, too, that the group composition remained intact through the four tasks, which condition might be expected to maximize the tendency for an individual to retain, throughout the entire session, the status he acquired as a leader on an early trial.

2. Further Analysis of the Information Variable

By removing the effects of these individual differences in general disposition to attempt to lead, we were able to obtain a more sensitive test of the situational variable that is of primary interest—possession of information—in that the effects of possession of information may have been obscured by individual differences. It has been shown previously that trials have little effect on the variance of the scores, and the effects of differences among the four tasks have been removed by standardizing the scores.

a. *Removal of individual differences.* It was possible, within the basic experimental design, to remove the effects of individual differences in disposition to attempt to lead. Since all individuals

were subjected to the same conditions with respect to possession or nonpossession of relevant information on Trials 1 and 2, it is assumed that differences which appear in these trials, other than those due to tasks and trials, are due to individual differences in "natural" predisposition to lead.

After attempted leadership scores (standardized for task differences) on Trials 1 and 2 were correlated with scores on Trials 3 and 4, regression equations were written to predict separately scores on Trials 3 and 4. These predicted scores represent that portion of the obtained scores that can be accounted for in terms of performance on Trials 1 and 2.

The multiple correlations of Trials 1 and 2 with Trials 3 and 4 were .52 ($R_{3.12}$) and .68 ($R_{4.12}$) respectively. The corresponding raw score regression equations are:

$$X_3 = .1776X_1 + .3497X_2 + 24.60$$

and

$$X_4 = -.0726X_1 + .7074X_2 + 18.97.$$

From these equations, predicted scores for Trials 3 and 4 were generated and subtracted from the obtained scores. These difference scores are independent of both task differences and individual differences.

b. *Analysis of difference scores.* In order to determine the effects of possession or nonpossession of relevant information on these scores, *t* tests involving the following comparisons were carried out:

1. The mean attempted leadership difference score of individuals possessing task-relevant information was compared with the mean attempted leadership difference score of individuals possessing no task-relevant information.

2. In the case of the Construction and Assembly tasks, the mean attempted leadership difference score of those in-

dividuals who had scored 0, 1, or 2 on the reading comprehension test, was compared with the mean attempted leadership difference score of those individuals who had scored 3, 4, or 5 on the test.

The first of these comparisons is regarded as the most general test of the effect of possession of relevant information on attempted leadership. The t ratio resulting from this comparison had a value of .92, which is not significant. Since this test did not reveal a significant effect, the second and more restricted comparison was carried out as a final means of determining whether possession of relevant information might be shown to affect attempts to lead.

It was reasoned that the absence of any effects due to possession of information might be due to an improper definition of "relevant" information. It was conceivable that the subjects had brought with them to the experiment information that was relevant to the tasks and that was not appreciably supplemented by the information made available during the course of the experiment. In that event, scores on the reading comprehension test would be a better indication of whether or not the subjects did, in fact, possess task-relevant information.

Accordingly, for the purposes of the second t test referred to above, possession of task-relevant information was defined in terms of performance on the reading comprehension test. Those who scored 3, 4, or 5 were classified as possessing information; those who scored 0, 1, and 2 were classified as not possessing information. A further restriction was imposed by testing only those attempted leadership scores observed on the Construction and Assembly tasks, since these were the only tasks for which differences in performance were found. The purpose of this restriction was to provide as critical a test as possible by limiting the test to those tasks for which differences in performance had been observed. Despite these considerations, the null hypothesis was accepted ($t = .53$).

In brief, it appears that *possession of task-relevant information, in and of itself, does not affect attempted leadership scores*. This is true even when we define possession of information in terms of reading comprehension test performance rather than in terms of the experimental manipulation of the information booklets.

IV. DISCUSSION OF RESULTS

The principal hypothesis of this study was not directly supported by the results of the experiment. Possession of information judged to be relevant to the solution of the groups' tasks was not shown to be a factor directly associated with greater frequency of attempts to lead. We have no evidence, therefore, to conclude that possession of task-relevant information per se is a major determinant of motivation for leadership.

Nevertheless, the results of the experi-

ment do point to important special conditions under which relevant information is associated with greater frequencies of attempts to lead. In the following discussion of the results we shall be concerned with four major issues: (a) the alternative uses to which task-relevant information may be put by group members; (b) the factors determining the relevance of information for group problem solution; (c) task characteristics associated with the frequency of attempts

to lead; and (d) changes in group behavior relative to the experiences of group members with information having varying task relevance.

It is believed that the discussion of these four issues will point to some of the reasons why the simple possession of task-relevant information cannot be used as a general predictor of attempted leadership.

A. ALTERNATIVE USES OF TASK-RELEVANT INFORMATION

The possession of task-relevant information may motivate an individual to assume the role of either "leader" or "resource person." Both of these roles were observed frequently among the subjects of the experiment. Both roles apparently were acceptable to the subjects. The fact that just prior to their problem-solving exercise they were exposed to and tested on information having some bearing on their tasks, may have suggested the potential value of the "resource person" role. In retrospect, it appears that failure to control feedback during the sessions may have been a major factor in facilitating a redistribution of information by a voluntary resource person. In the experimental instructions, no emphasis was placed on leadership or organization of group effort, and rewards were offered for group performance rather than for successful leadership. It has been noted, also, that the groups were composed of relatively homogeneous and previously unacquainted individuals. These conditions, too, may have helped to define the particular situation as one in which there was no special incentive to try to assume a leader role in preference to that of a resource person.

Another deviation from the direct use of relevant information to lead appeared when one group member engaged in extracting relevant information from another. On occasion, a dominant individual, or one who seemed previously to have gained some status as a leader, would ask for and receive task-relevant information from another group member and then proceed to utilize this borrowed information in attempting to lead. In one sense, the group member who supplied the information was a "resource person." But he differed from those who spontaneously volunteered their information to the group in that he did not seem consistently to see himself in the "information giver" role. Examination of transcripts of the sessions makes it apparent that the relationship between posses-

sion of information and member function in the group process can be highly complex.

B. FACTORS DETERMINING THE RELEVANCE OF INFORMATION

The effectiveness of the experiment was compromised in part by results that raise serious questions about the degree to which we were able to manipulate the relevance of information variable. We were able to achieve high agreement among judges who appraised each of the eight sets of information, with respect to which set within a given pair was more relevant to the task described. In addition, it was demonstrated that those individuals who had been provided with information judged to be relevant achieved significantly higher scores on both the comprehension and the retention tests.

In the more critical test, however, of whether the information was relevant in the sense that it contributed to better group task performance, significant results were obtained for only two of the tasks. In fact, in the case of the Discussion task, there was a slight tendency for the groups given the nonrelevant information to do better than those possessing the so-called relevant information. Although this discrepancy is a minor trend in a small sample of observations and might be dismissed on this basis alone, there may be something to be gained by re-examining the concept of relevance of information.

Perhaps the concern with the relation between possession of information and actual group task performance is misplaced. We were interested primarily in the manipulation of information in order to examine the possibility of associated variation in frequency of attempting to lead. Presumably, any such association would be mediated through the perception by the group members of the relevance of their own information to the solution of the group's problem. If this were the case, then it might be reasoned that relevant-appearing information would be more likely to stimulate attempts to lead than information that was not obviously relevant, even if it proved to be useful in practice. A fault in this reasoning, however, may lie in the contrary effects of accumulated experience with so-called relevant information that actually was found to be of no use. Unfortunately, the information supplied in the case of the Discussion task seemed to be largely of this type. Many facts about voting behavior were supplied by the booklet of relevant information. Only three or four of these facts could be employed in making a rational estimate of the number of votes cast in Oregon for the Republican candidate for president in 1940. The other

facts, although relevant to the prediction of voting behavior in general, had nothing to do specifically with Oregon, or the year 1940, or Republican voting. Hence, these items may have added only confusion.

In the cases of the two tasks, Assembly and Construction, for which a significant relation was found between possession of the relevant information and task performance, a greater proportion of the information was directly usable. A good performance on the Construction task was virtually impossible without knowledge of the manufacturer's diagram against which the group product was scored. Over three-quarters of the specific items of information supplied for the Assembly task were needed for a perfect performance. Although for both of these tasks some surplus information was supplied, there appeared to be a relatively small amount that could be regarded as misleading.

Another aspect of the problem of the manipulation, or distribution, of relevant information related to the novelty for the group members of the various tasks. The screening of subjects did eliminate from the experiment individuals who had had specific experience with obviously similar tasks. Nevertheless, the four tasks varied greatly in terms of the degree to which "everyday common sense" might appear to afford an adequate basis for solution of the problem. The Strategy and Discussion problems seemed most frequently to be approached with efforts to use previously acquired knowledge and experience. The Assembly task probably was least familiar to most of the subjects, although occasionally some incidental knowledge (acquired through tinkering with the family radio) was brought to bear on this problem.

Some of the findings of the analysis involving task and information sequences and their interactions may be more understandable in view of the special characteristics of the four tasks and the complex relations among tasks, information, and attempts to lead.

C. TASK CHARACTERISTICS ASSOCIATED WITH THE FREQUENCY OF ATTEMPTING TO LEAD

The largest single source of variation among the attempted leadership scores was task differences (See Section III, B, 1, 2). In other words, special characteristics of the tasks had much to do with how frequently group members tried to lead. By far the largest frequency of attempts to lead was observed when the groups were working on the Strategy task, which required the four group members to work as a team in competition with the experimenter in a modified game of peggy. Before each move, the four group members were required to de-

cide upon the deployment of their four pegs. This rule, in turn, meant that the group had to reach consensus with each move. Therefore, the interaction among the group members was lively and characterized by a large number of attempts to lead. Polite reticence quickly disappeared in the face of direct, positive recommendations of hastily conceived plans that were just as frequently rejected by other members' equally hearty endorsement of some other plans. The pressure of time and the recurring necessity of arriving at consensus elevated the frequency of attempted leadership acts above that observed for any other of the four tasks.

In contrast to the rapid interaction and full participation that were noted in the Strategy problem, there was relatively little interaction in the Discussion problem. The typical sequence of events included a short general discussion at the beginning of the fifteen minute period, followed by an interval during which individuals worked alone on various computations, then engaged in some comparing of results, and, finally, near the close of the allotted time, a short period of active participation by all. At this point, agreement was reached upon a final estimate, the only time at which consensus was required.

The Construction task differed from the others primarily in the kind of atmosphere created for the group. Work on this task involved much gross muscular activity with the members helping each other to manipulate the materials. Since it was quite easy for all group members to work simultaneously on different parts of the task, participation started and remained at a high level throughout the period. The type of activity required and the amount of participation stimulated both served to make this task an excellent "ice-breaker" for previously unacquainted individuals. These characteristics of the task may have accounted for the fact that more leadership acts were attempted by members of groups who began their sequence of four tasks with the Construction problem.

The Assembly task seemed to be extremely frustrating for group members who had not received relevant information. Occasionally, a rather prolonged period of silence was noted at the beginning of such a session. Participation was moderately high, however, and some agreement was required in order that a particular component could be placed in the circuit. Such informal observations have made it increasingly obvious to us that much more research is needed on task characteristics per se as predictors of leadership behavior in groups.

D. CHANGES IN GROUP BEHAVIOR

In the analysis of the experimental data we noted the significant effects on attempted leader-

ship scores of two conditions: (a) whether the group began its initial task with or without relevant information, and (b) the sequence in which the four tasks were presented. Although this is not the only difference between the sequences, this sequence effect may be regarded as a function of the initial task in the sequence. If we do so regard the sequence and also note the effect of information on the initial trial, we can infer that initial group experiences may have modified subsequent attempts to lead.

Following the above interpretation of these effects, we find that the possession of relevant information on the initial trial does tend to influence attempts to lead on subsequent trials. For the groups that had no relevant information on Trial 1, the mean attempted leadership score for Trials 2 through 4 is 3.38, to be compared with a mean of 4.03 for groups with relevant information on the first trials. Moreover, those group members who began their tasks with either the Construction or Assembly problem had an average attempted leadership score of 4.02 for the subsequent three tasks, to be compared with a mean score of 3.39 for those members whose initial experience was with the Discussion or Strategy tasks. It will be recalled that the latter two tasks were those for which the supplied information was of less clear-cut relevance to task performance. It seems evident, therefore, that the possession of useful information on the initial task of the series tended to raise the succeeding frequency of attempts to lead. Even in a brief period of fifteen minutes some group norm or set that affected this type of group member behavior appears to have been established.

E. IMPLICATIONS

The experiment was designed to test a hypothesis derived from central theorems of a theory of leadership and group behavior (23, Appendix A).

The experimental hypothesis stated in the null form was as follows: In a mutual problem-solving situation the frequency of attempts to lead by those individuals possessing task-relevant information is not significantly different from the frequency of leadership acts attempted by individuals possessing nonrelevant information. At the time the hypothesis was formulated, we assumed that initial possession of task-relevant information would insure that the individual would be able to see a clear relation between this relevant information and the solution of the experimental tasks (mutual problems). Therefore, his estimate of the probability of the act's leading to mutual problem solution would be higher than if he did not possess relevant information. The hypothesis still appears to us

to be related to the theorem, but it now seems likely that control was not maintained over certain other variables stipulated by the theory as also being related to attempted leadership.

For example, we attempted in the initial assembly of the groups, to control variables related to individual member differences (i.e., . . . 2, his estimate of the probable effect of the act on the potential of the group for need-disposition satisfaction, 3, the degree of dissatisfaction he feels with the mutual problem, and 4, the strength of his relevant need dispositions). These controls were provided through offering uniform incentives for excellence of performance, selection of group members from a relatively homogeneous sample (i.e., male introductory psychology students, who passed our screening questions), an insistence upon members' being unacquainted before the experiment, and the use of standard experimental procedures. Although some variance may have been introduced by the inadequacies of these controls, we have no reason to believe that these were the major problems.

The results of the experiment and our informal observations both point to changes occurring in the groups themselves during the experimental sessions as the chief uncontrolled source of variation. Thus, the controls introduced at the outset do not appear to have been maintained throughout the experiment. During the initial period of their work together, the subjects rapidly became something other than unacquainted persons, developed more or less esteem in the group, learned that certain of the facts they possessed were useful and that others were not, etc. We have noted evidence suggesting that the status of the information variable may have changed during the experiment. After a brief period of group interaction, the relation of initial possession of relevant information to the individual's "probability estimate" becomes increasingly complicated.

A group member's estimate of the probability that his contemplated leadership act will result in problem solution appears to undergo revision influenced by his experience in attempting to use his "relevant" information. Such shifting was not controlled. Instead, an implicit assumption had been made that no shift would occur. In effect, therefore, we had chosen the alternative of maintaining an indirect rather than a simple and direct relation between the hypothesis tested and the proposition stated in the theory. This relationship is indirect, or qualified, in the sense that the hypothesis, with reference to a number of variables theoretically specified as pertinent to attempted leadership, has assumed that such variables would be assigned the status of "other things being equal." The experiment appears to have provided a test of the hy-

pothesis as stated, but the evidence at hand does not seem to warrant revision of the theory itself. The experience with this attempt to test the propositions of the theory suggests a number of variables that must be taken into account in small group experiments—particularly serial, time, and feedback effects. We can point to the dangers involved in assuming that small groups

of laboratory subjects will remain without a "tradition" for even very short periods of time. The research worker concerned with investigating the behavior of small groups cannot assume that, once he has manipulated his independent variable, his manipulation will remain in effect throughout his experiment.

V. SUMMARY AND CONCLUSIONS

In order to investigate the relationship between possession of task-relevant information and attempted leadership behavior, sixteen groups of four male subjects each were given four group problems—Assembly, Strategy, Construction, and Discussion tasks—to solve. Task-relevant information was supplied to various members of each group on each task during a training period preceding the groups' attempts to solve the problems. Immediately following the training period, and again after the completion of the experimental session, each group was tested to determine the extent to which the group members retained the task-relevant information supplied them. During the experimental session three trained observers located behind one-way vision mirrors tallied the frequency of occurrence of attempted leadership acts. When available, a "naive" observer rated members of the groups on "amount of leadership."

Interobserver reliability of the observations of attempted leadership acts was estimated by means of the intraclass correlation coefficient (r'). The intraclass correlation among the three trained observers was .83. For the three possible pairs of observers, the intraclass correlation coefficients were .80, .81, and .88. These values indicate that trained observers are able to recognize and record attempted leadership acts with a high degree of consistency.

The ratings of the naive observers on

amount of leadership were correlated with the number of attempted leadership acts reported by the three trained observers. This product-moment correlation coefficient was .47. The magnitude of this correlation indicates that, while there is some communality between the concept of the attempted leadership act and the layman's notion of leadership, the two concepts are largely independent.

As a check on the meaningfulness of the experimental classification of information as more or less task-relevant, six individuals not associated with the research group were asked to judge the information given the subjects. For three tasks, there was unanimous agreement among the judges as to how the information should be classified. The probability that such agreement would occur by chance alone is less than one in a thousand. On the Assembly task five of the six judges agreed with the experimental classification of the information. This level of agreement would be expected to occur by chance less than four times in one hundred.

As another means of assessing the distribution of information, the subjects were tested on the task-relevant material both immediately after the training period and also following the completion of their tasks. In each instance, it was found that subjects who had had access to more task-relevant information scored significantly higher on the tests than those who had been presented with

nonrelevant information. Insofar as performance on multiple-choice item tests is concerned, the distribution of information among the experimental subjects was in accordance with the experimental design.

Task performance was not so clearly related to the information variable. On two of the tasks (Assembly and Construction) individuals who possessed relevant information scored significantly higher than those who did not possess such information. On the other two tasks, Strategy and Discussion, there were no significant differences between the mean performance scores of the two classes of subjects. On the latter task, however, there was a slight trend in a reversed direction, i.e., a tendency for those subjects who possessed nonrelevant task information to receive superior task performance scores.

The principal hypothesis under investigation in this experiment is that in a group problem-solving situation individuals who possess task-relevant information will attempt a greater number of leadership acts than individuals who possess no task-relevant information. The results of the experiment do not support this hypothesis. However, certain interaction effects involving information were found to be significantly related to attempts to lead.

Three interactions—two first-order and one second-order—involving the information variable were found to be significant. These were (a) Information \times Initial Information, (b) Information \times Task Sequence, and (c) Information \times Initial Information \times Task Sequence. The fact that the Information \times Initial Information interaction was significant indicates that possessing (or not possessing) information has an effect upon attempted leadership scores that depends

upon whether or not the subjects were supplied with relevant information on their *initial* task in the sequence of four tasks. The largest average attempted leadership score was obtained in the case of subjects who had begun their sequence of four tasks *with* relevant information, but who worked *without* relevant information on one or more of the succeeding task. (See Section III,B,1,c for a more complete statement of this interaction effect.)

The Information \times Task Sequence interaction was found to be significant. This is due to the departure from the expected attempted leadership scores when groups began their experimental sessions with the Discussion task. Under this condition, attempted leadership scores were higher for those individuals who did not possess relevant information. This reversal appears to be related to the similar reversal observed in the case of performance scores on this task. Apparently the information that was experimentally classified as "relevant" to the Discussion task served to confuse the subjects. Accordingly, initial experience with seemingly relevant information that actually was of little use to the group served to depress the ensuing level of attempted leadership acts for such groups.

No clear-cut interpretation of the complex effects present in the second-order interaction of Information \times Initial Information \times Task Sequence is readily at hand. This interaction, taken in conjunction with the preceding interaction effects, indicates that while information has no simple and direct effects upon attempted leadership, it is related to other situational variables.

The principal conclusions drawn from the results of this experiment may be summarized as follows:

1. Under the conditions of the ex-

periment, possession of task-relevant information was shown to have a complex rather than a simple relation to the frequency of attempts to lead.

2. The number of leadership acts attempted by group members during the fifteen minutes allotted for the solution of each of four different tasks differed

significantly among the tasks.

3. Individuals displayed a significant, but moderate, consistency in their disposition to attempt leadership.

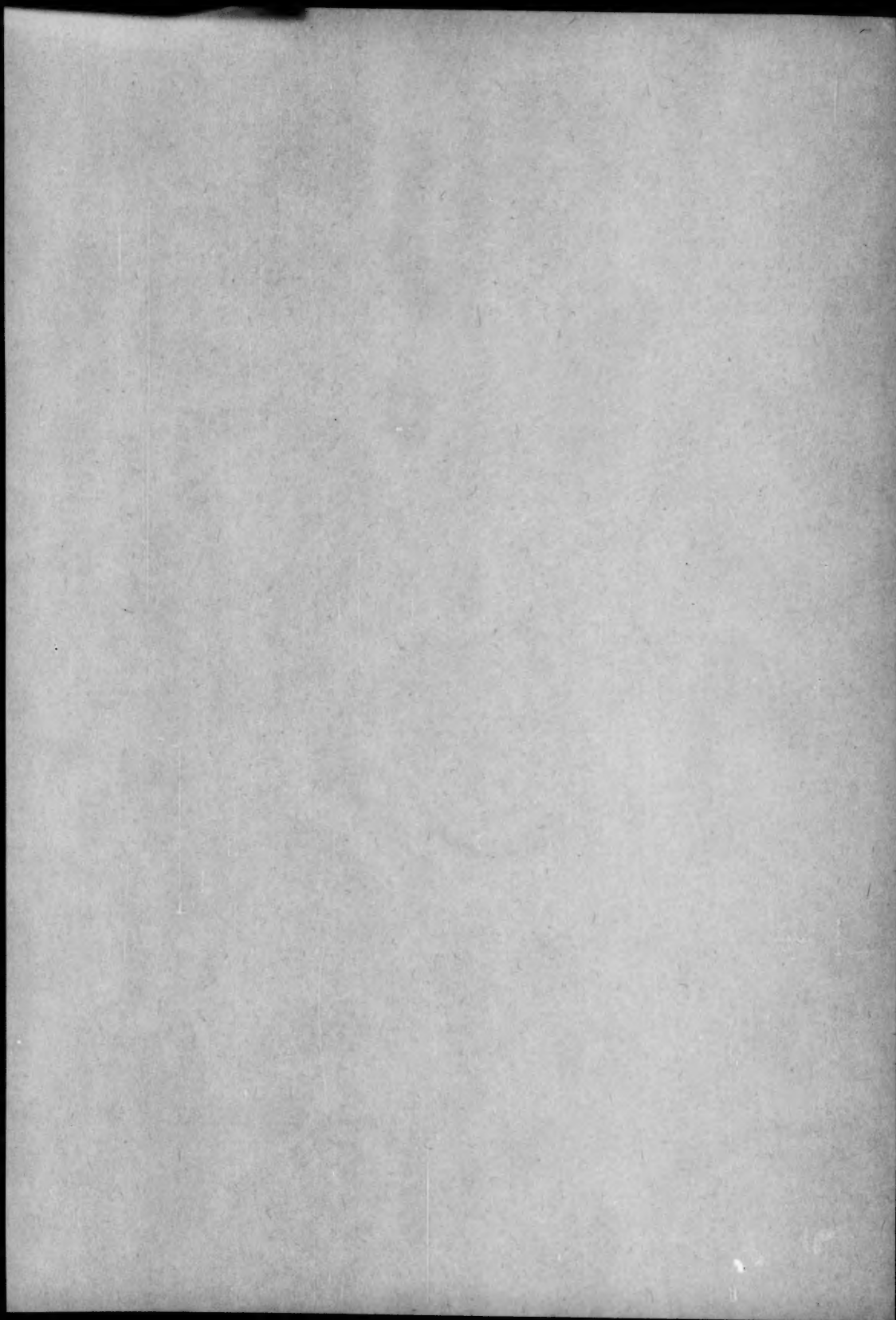
4. Observers can be trained to make reliable observations of attempted leadership acts.

REFERENCES

1. ANSBACHER, H. L. The history of the leaderless group discussion technique. *Psychol. Bull.*, 1951, **48**, 383-390.
2. BALES, R. F. *Interaction process analysis*. Cambridge: Addison-Wesley, 1950.
3. BASS, B. M. An analysis of the leaderless group discussion. *J. appl. Psychol.*, 1949, **33**, 527-533.
4. BASS, B. M., WURSTER, C. R., DOLL, PADDY ANN, & CLAIR, D. G. Situational and personality factors in leadership among sorority women. *Psychol. Monogr.*, 1953, **67**, No. 16 (Whole No. 366).
5. BELL, G. B., & FRENCH, R. L. Consistency of individual leadership position in small groups of varying membership. *J. abnorm. soc. Psychol.*, 1950, **45**, 761-767.
6. CALDWELL, O. W. Some factors in training for leadership. *Nat. Asso. Sec. Sch. Prin., Fourth Yearb.*, 1920. Pp. 2-13.
7. CARTER, L., HAYTHORN, W., MEIROWITZ, B., & LANZETTA, J. A note on a new technique of interaction recording. *J. abnorm. soc. Psychol.*, 1951, **46**, 258-260.
8. CARTER, L., HAYTHORN, W., MEIROWITZ, B., & LANZETTA, J. The relation of categorizations and ratings in the observation of group behavior. *Hum. Rel.*, 1951, **4**, 239-254.
9. CARTER, L., HAYTHORN, W., SHRIVER, B., & LANZETTA, J. The behavior of leaders and other group members. *J. abnorm. soc. Psychol.*, 1951, **46**, 4, 589-595.
10. DEUTSCH, M. An experimental study of the effects of cooperation and competition upon group process. *Hum. Relat.*, 1949, **2**, 199-231.
11. DEUTSCH, M. A theory of cooperation and competition. *Hum. Relat.*, 1949, **2**, 129-152.
12. DEUTSCH, M. *A study of conditions affecting cooperation*. Annu. Rep., No. 2, Contract No. NR-285 (10), Human Relations Branch, Office of Naval Research, 1954 (mimeographed).
13. FISHER, R. A. *Statistical methods for research workers*. London: Oliver and Boyd, 1938.
14. GIBB, C. A. The principles and traits of leadership. *J. abnorm. soc. Psychol.*, 1947, **42**, 267-284.
15. GIBB, C. A. The research background of an interactional theory of leadership. *Australian J. Psychol.*, 1950, **28**, 10-42.
16. GIBB, C. A. An experimental approach to the study of leadership. *Occup. Psychol.*, 1951, **25**, 233-248.
17. GUETZKOW, H. Utilizing and categorizing problems in coding quantitative data. *J. clin. Psychol.*, 1950, **6**, 47-58.
18. GUETZKOW, H. (Ed.) *Groups, leadership and men*. Pittsburgh: Carnegie Press, 1951.
19. HALPIN, A. W., & WINER, B. J. *The leadership behavior of the airplane commander*. Washington, D.C.: Human Resources Research Laboratories, Dept. of the Air Force, 1952.
20. HEISE, G. A., & MILLER, G. A. Problem solving by small groups using various communication nets. *J. abnorm. soc. Psychol.*, 1951, **46**, 327-336.
21. HEMPHILL, J. K. Situational factors in leadership. *Ohio State Univer. Bur. Educ. Res. Monogr.*, 1949, No. 32.
22. HEMPHILL, J. K., & COONS, A. E. *Leader behavior description*. Columbus: Personnel Research Board, The Ohio State Univer., 1950.
23. HEMPHILL, J. K., PEPINSKY, PAULINE N., SHEVITZ, R. N., JAYNES, W. E., & CHRISTNER, CHARLOTTE A. *Leadership acts: 1. An investigation of the relation between task relevant information and attempts to lead*. Columbus: Ohio State Univer. Res. Found., 1954.
24. HEMPHILL, J. K., & WESTIE, C. M. The measurement of group dimensions. *J. Psychol.*, 1950, **27**, 325-342.
25. HEYNS, R. W., & ZANDER, A. F. Observation of group behavior. In L. Festinger and D. Katz (Eds.), *Research methods in the behavioral sciences*. New York: Dryden, 1953. Pp. 381-417.
26. JENKINS, W. O. A review of leadership studies. *Psychol. Bull.*, 1947, **44**, 54-79.
27. JOHNSON, P. O. *Statistical methods in re-*

- search. New York: Prentice-Hall, 1949.
28. KATZ, D. Social psychology and group processes. *Annu. Rev. Psychol.*, 1951, **2**, 137-172.
29. LEAVITT, H. J. Some effects of certain communication patterns on group performance. *J. abnorm. soc. Psychol.*, 1951, **46**, 38-50.
30. NEWCOMB, T. M. Motivation in social behavior. In J. S. Brown, et al., *Current theory and research in motivation: a symposium*. Lincoln: Univer. of Nebraska Press, 1953. Pp. 139-161.
31. ROSEBOROUGH, MARY E. Experimental studies of small groups. *Psychol. Bull.*, 1953, **50**, 275-303.
32. SHARTLE, C. L., & STOGDILL, R. M. Methods of determining patterns of leadership behavior in relation to organizational structure and objectives. *J. appl. Psychol.*, 1948, **32**, 286-291.
33. SMITH, M. B. Social psychology and group processes. *Annu. Rev. Psychol.*, 1952, **3**, 175-204.
34. STOGDILL, R. M. Personal factors associated with leadership: a survey of the literature. *J. Psychol.*, 1948, **25**, 35-71.
35. STOGDILL, R. M. Leadership, membership, and organization. *Psychol. Bull.*, 1950, **47**, 1-14.

(Accepted for publication October 16, 1955)



GEORGE BANTA COMPANY, INC., MENASHA, WISCONSIN